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# **Welfare Reform, the Business Cycle, and the Decline in AFDC Caseloads**

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The past decade has witnessed unprecedented changes in both the caseload size and administration of the Aid to Families with Dependent Children (AFDC) program. Welfare caseloads increased nationwide by about 26 percent from 1990 to the peak in early 1994, and then declined by 35 percent as of the third quarter of 1998. With the lone exception of Hawaii, every state has experienced caseload reductions, ranging from a 13 percent drop in Alaska to an 86 percent drop in Wisconsin. Two factors are widely credited for these declines: strong economic growth and a fundamental transformation of the welfare system (Blank 1998; Council of Economic Advisers 1997; Ziliak et al. 1997). Since 1993, nearly 18 million new jobs have been created, unemployment rates have fallen to their lowest level in a generation, and employers of low-wage workers are arguably facing the tightest labor market in 50 years (Maharaj 1998). During this same period, the U.S. Department of Health and Human Services (HHS) became more liberal in granting waivers from federal AFDC requirements, permitting states to experiment in earnest with their welfare systems. These experiments culminated in the passage of the Personal Responsibility and Work Opportunity Reconciliation Act of 1996 (PRWORA). PRWORA created a new federal block-grant program called Temporary Assistance for Needy Families (TANF) to replace AFDC; the new program eliminates individual entitlement to cash assistance and gives states wide latitude in setting program parameters.

While it is generally incontrovertible that the business cycle and welfare reform underlie the dramatic decline in the AFDC program, the relative contribution of each factor is in dispute. While the Council of

Economic Advisers (CEA) presents a number of specifications, their preferred estimate suggests that the business cycle accounts for 44 percent of the decline in AFDC caseloads from 1993–1996, while welfare waivers account for 31 percent of the decline. Alternatively, Ziliak et al., using higher-frequency data and a more dynamic specification, attribute nearly two-thirds of the decline to the robust economy but nearly nothing to welfare reform.

Because the policy implications from the CEA and Ziliak et al. differ significantly, it is important to delineate the methodological differences in order to make more informed fiscal and welfare policy. For example, if AFDC cases only weakly respond to business-cycle conditions, then we would expect the welfare-program budget surpluses that many states have enjoyed recently to persist even into a recessionary period. Alternatively, if caseloads are strongly countercyclical, then states that have failed to save for a “rainy day” may face difficult fiscal constraints during the next cyclical downturn. In addition, any interrelationship between welfare reform and the macroeconomy may become disentangled when the economy turns toward recession. Bishop (1998) presented evidence that most of the increase in labor force participation rates since 1994 is among single women with children. If a large share of these women are former welfare recipients and the recent success of welfare reform is tied to the robust economy, then the movement from welfare to work could be much weaker in a sluggish economy.

Our purpose here is threefold. First, we conduct an extensive reconciliation between the findings in Ziliak et al. and those of the CEA.<sup>1,2</sup> Specifically, using the data and sample period employed by the CEA, we examine the relative impacts of the business cycle and welfare reform on the 1993–1996 caseload decline via numerous modeling choices, including using recipients versus cases as the outcome of interest, using year dummies versus a cubic trend to control for macroeconomic factors, using controls for welfare benefits, using weights in the regression model, using different sample periods, using first differences instead of levels, and using a dynamic framework. Second, we turn our attention to the issue of how welfare reciprocity might respond in the event of a recession. To address this question, we employ the preferred dynamic specifications that arise from the reconciliation and simulate how caseloads respond to alternative “shocks” to the unem-

ployment rate. Third, we examine the possibility of interactions between the macroeconomy and welfare reform.

Our reconciliation suggests that the differences in results between Ziliak et al. and the CEA emanate largely from the treatment of dynamics. These dynamics surface in the form of sluggish adjustment of current caseloads to past caseloads, from lags in the response of caseloads to changes in unemployment rates, and from nonstationarities in caseloads (especially at monthly frequencies). The primary consequence of controlling for caseload dynamics is to reduce the role of welfare reform relative to the macroeconomy in generating the decline in AFDC caseloads. Once we control for dynamics, we attribute up to 75 percent of the 1993–1996 caseload decline to the macroeconomy and at most 1 percent to welfare reform.

Moreover, the simulations underscore both the importance of controlling for dynamics and the cyclical sensitivity of welfare reciprocity. We find that the implied long-run effect of a 1-percentage-point increase in the unemployment rate is 2.5 and 6 times the static estimate in levels and first differences, respectively. In addition, we find that a 2-percentage-point increase in the unemployment rate leads up to an 11.7 percent increase in welfare reciprocity after four years, while a 4-percentage-point increase yields a 23.4 percent increase in reciprocity. Finally, the results from interactions between the macroeconomy and welfare reform indicate that pre-TANF welfare reform requires a robust economy (i.e., low unemployment rates) in order to have a negative impact on reciprocity rates.

## REVIEW OF CASELOAD LITERATURE

Research on modeling aggregate AFDC caseloads is a relatively recent addition to the welfare literature. This stands in contrast to the large microeconomic literature that focuses either on the determinants of AFDC participation or the duration of welfare spells (Danziger, Haveman, and Plotnick 1981; Moffitt 1992). A few previous papers have considered the impact of economic stimuli on caseload levels without examining the concurrent effects of welfare reform.<sup>3</sup> The purpose of most of these studies has been to develop models that

can accurately forecast changes in the number of families receiving AFDC over time. They tend to use time series data and focus on a single state, and in some cases on a single city (New York). The study by Peskin, Tapogna, and Marcotte (1993) for the Congressional Budget Office is a notable exception in their application of quarterly time-series data for national AFDC-Basic and AFDC-UP (unemployed parents) caseloads.<sup>4</sup> They employed a distributed lag model, permitting the business cycle to have a dynamic impact on caseloads, and found that both Basic and UP caseloads exhibit strong countercyclical movements. Specifically, their model predicts that a 1-percentage-point increase in the employment gap (the percentage difference between potential and actual employment) leads to a 0.5 percent decline in Basic and a 1.7 percent decline in UP caseloads in that quarter.

Moffitt (1987) used cross-sectional and time-series data separately to study the large run-up in AFDC reciprocity in the late 1960s. For the cross-sectional analysis, he employed a static model of AFDC participation for 1967, 1973, and 1979, where participation is a function of measured demographics, AFDC program parameters, and the unemployment rate. The model predicts that most of the run-up is unexplained by economic and demographic factors. Instead, Moffitt attributed the increase to non-economic factors, such as court-ordered and legislative decisions that made eligibility easier and an increased willingness to participate in the program, possibly due to a reduction in the stigma associated with benefit receipt.

More recently, researchers have turned to state-level administrative data to model the impact of both the business cycle and welfare reform on AFDC caseloads. The advantage of the state panel-data approach is that it fosters identification of the business-cycle and welfare-reform effects by exploiting spatial differences across states and time-series differences within states. Because the focus of this paper is in reconciling the results from this literature, we provide a more detailed summary of the methods.

The Council of Economic Advisers (1997) employed annual state-level panel data for 1976–1996 to model per capita AFDC reciprocity rates. The dependent variable combines AFDC recipients, not cases, in both the Basic and UP programs; this implicitly assumes that the business-cycle and welfare-reform responses between the two groups are identical. The CEA modeled per capita reciprocity as a function of the

business cycle, waivers from federal welfare programs, program parameters, and unobserved state fixed effects and trends. To capture the impact of the business cycle, the CEA used current and one-period-lagged state unemployment rates; the lag controls for any delays between the timing of unemployment and the receipt of welfare. The waiver variables were defined as the fraction of the year that the (full-state) waivers are approved. In some specifications the waivers were aggregated as “any statewide waiver,” while in others they were disaggregated as “JOBS sanctions,” “time limits,” “work requirements,” “family cap,” and “earnings disregards.” In several specifications, the CEA included both contemporaneous waiver variables and one-period “lead” waivers. The latter was an attempt to control for political rational expectations on the part of welfare recipients, signifying that welfare benefits were soon to be threatened. The AFDC program parameter was the AFDC maximum-benefit guarantee for a family of three, which is used to capture the “price” of welfare. Lastly, the state fixed effects and trends controlled for permanent differences in labor-force composition and welfare populations, as well as trending differences across states.

In their preferred results (Council of Economic Advisers 1997, Table 2, Column 6), the CEA found that contemporary unemployment has little effect on AFDC reciprocity, but that a 1-percentage-point increase in lagged unemployment increases reciprocity by almost 5 percent. The waiver effects are mixed—there is no significant current effect from the variable “any statewide waiver,” but the lead effect suggests that states with any anticipated federal waiver could expect a 6 percent decline in annual reciprocity rates. The conclusions were reversed for the “JOBS sanctions” variable: the current effect suggests a 7 percent decline, but there is no significant lead effect.

The specifications in Blank (1998) are similar to the CEA’s, with a few notable exceptions. She used annual state panel data from 1979–1995 and estimated separate models for AFDC-Basic and AFDC-UP caseload levels. In addition to the explanatory variables used by the CEA, Blank controlled for a second lag in unemployment, along with interstate differences in median and 20th percentile wages, racial composition, female headship, age composition, average years of schooling, and political affiliation of the governor and state legislature.

Overall, Blank's estimates of the effects of the business cycle and welfare waivers do not differ substantively from the CEA's (see Blank 1998, Table 2, Column 1). The net effect of a 1-percentage-point increase in unemployment leading to a 3.8 percent increase in cases is comparable to the CEA's estimate of 4.07 percent. When Blank included a lead waiver (Blank 1998, Table 3, column 2) she estimated that "any statewide waiver" leads to about an 12.8 percent reduction in caseloads; again, this is similar to the CEA's overall estimate of about 12.5 percent (summing up the current and lead coefficients). It is important to note how closely the results mimic each other even though Blank's pertain to Basic cases while the CEA's pertain to total recipients. Blank did find that the UP program is more responsive to macro-economic and policy variables than the Basic program; however, since UP cases are only about 6 percent of the total, pooling the samples adequately represents the majority of cyclical and welfare-reform movements in cases. Unlike the CEA, Blank did not prefer the models with lead waiver variables, because the latter likely capture other factors changing in the states (prior to waiver approval) and not true program effects.

The paper by Ziliak et al. (1997) differs in several dimensions from those of the CEA and Blank. The period under study was shorter (1987–1996), the data were monthly as opposed to annual state-level data, and the empirical specification was more parsimonious in control variables because there is very little within-year variation in measured demographics. The dependent variable was per capita AFDC-Total caseloads, while the measures of the business cycle were either employment per capita or the unemployment rate. The welfare waiver dummy variables (1 = month waiver was approved) were broken down into four categories: work requirements, time limits, work pays (e.g., higher earnings disregard), and responsibility (e.g., family cap). Finally, there were controls for state fixed effects and time trends, as well as month-of-year dummy variables to control for seasonality in caseloads and employment.

Two additional methodological differences in Ziliak et al. are that the model was estimated in first differences and it had a richer dynamic structure. Ziliak et al. provided evidence that monthly caseloads are nonstationary in levels but stationary in first-differences. Moreover, they introduced dynamics into the model in the form of state depen-

dence (6 lags of the dependent variable), lagged business cycles (11 lags of employment per capita), and an “implementation lag” for the welfare waivers. The implementation lag was defined as the number of months since approval and was designed to capture the fact that it may take several months or even years to revamp the program with the reforms in place.

The results and subsequent policy implications of Ziliak et al. differ markedly from those in the CEA and Blank. Ziliak et al. gave much more weight to the business cycle relative to welfare reform in explaining the recent decline. The small overall effect of welfare waivers arises because the type of waiver that a state adopts matters for aggregate caseload levels: some are caseload-decreasing while others are caseload-enhancing. In simulations of the dynamic model, they showed that work requirements and responsibility waivers significantly reduce caseloads, yet waivers that make work more attractive increase caseloads by nearly the same percentage. Ziliak et al. conducted a limited reconciliation of their results with those of the CEA and Blank and concluded that the key difference arose through the modeling of dynamics.

In the following sections, we expand on the reconciliation begun in Ziliak et al. One issue that we do not address econometrically is the effect of lead waivers. As mentioned above, Blank did not believe that the lead variables signal true program effects. Martini and Wiseman (1997) went even further in their critique of the CEA’s use of lead effects, arguing that many of the waivers are not “threatening” per se and the one waiver that might be perceived as threatening, “JOBS sanctions,” has no significant lead effect. Moreover, they claimed that it is unlikely that welfare recipients would respond one year in advance of waiver approval when the approval date is so uncertain, and that if lead effects are to be interpreted literally, then all states without waivers as of August 1995 should be coded with a lead effect anticipating PRWORA. However, the latter would be unreasonable, because passage of PRWORA was uncertain up to a month before President Clinton signed it into law. Additionally, Ziliak et al. presented evidence that lead effects disappear in annual first-difference models. Because of these limitations, we do not explore the role of lead waivers further. Consequently, our reconciliation focuses on the CEA’s specification 2, which does not control for lead waiver effects.



## RECONCILING THE CEA AND ZILIAK ET AL.

The data used in this study are the same as those employed by the CEA. Although Ziliak et al. used state-level monthly data, for the purposes of the reconciliation it is most instructive to use the same data as the CEA in order to abstract from data issues and focus on modeling choices. The annual state-level panel data are for the 1976–1996 federal fiscal years and contain information on AFDC recipients, state unemployment rates, state population, real AFDC maximum benefits for a family of three, and statewide welfare waivers. In addition to the CEA data, we collected information on caseload levels by state and year. The reader is referred to the CEA technical report (1997) for more extensive details about the data.

We begin our reconciliation by replicating specification 2 from Table 2 of the CEA. This static model regresses the log of AFDC recipients per capita on the unemployment rate, the real maximum AFDC benefit for a family of three, year dummies, and state-specific fixed effects and trends. The year dummies control for macroeconomic factors that affect all states in a given year, such as federal expansions in the Earned Income Tax Credit or oil-price shocks. Meanwhile, the controls for time-invariant state fixed effects and for state-specific trends are intended to capture not only fixed unobserved state-specific propensities to take up welfare, but also slow-moving state-specific trends in demographics such as fertility rates, marital status, and migration patterns. Thus, the static model for each state  $i$  ( $i = 1, \dots, 51$ ) in period  $t$  ( $t = 1, \dots, 21$ ) is

$$\text{Eq. 1} \quad R_{it} = \mu + \alpha UR_{it} + \beta W_{it} + \theta B_{it} + \gamma_t + \delta_i + \lambda_i t + \varepsilon_{it},$$

where

$R_{it}$  = the natural log of per capita AFDC recipients

$UR_{it}$  = the unemployment rate

$W_{it}$  = the welfare reform indicator that equals the fraction of a year (based on the approval date) that “any statewide waiver” is in effect

$B_{it}$  = the real maximum AFDC benefit for a family of three

$\gamma_t$  = a vector of year effects

$\delta_i$  = the time-invariant state-specific deviation from the overall constant  $\mu$

$\lambda_{it}$  = the state-specific trend,

$\varepsilon_{it}$  = a random error.

To control for possible heteroskedasticity, the regression is weighted by state population.

In column 1 of Table 1, we present the base-case, weighted-least-squares estimates of the effects of the business cycle and welfare reform on per capita AFDC recipients (CEA's specification 2). The results suggest that a 1-percentage-point increase in the unemployment rate will yield a 3.1 percent increase in per capita AFDC recipients. Alternatively, states with a statewide welfare waiver experience a 5.5 percent decline in AFDC reciprocity relative to states without a waiver.<sup>5</sup> The point estimates are useful for decomposing the fraction of the 1993–1996 decline in recipients attributable to the business cycle and to welfare reform. The estimates indicate that the robust economy accounted for 31 percent of the decline, while welfare reform accounted for 16 percent.

### Recipients versus Caseloads

The first step towards reconciling the results from Ziliak et al. with those from the CEA involves the choice of dependent variable. Ziliak et al. used AFDC caseloads per capita as the dependent variable, rather than the number of AFDC recipients. Cases may be preferred to recipients because the latter confounds the number of households receiving AFDC with the within-household fertility behavior. In addition, the number of cases may better represent the underlying household behavioral response to changes in economic conditions and welfare reform, because it is the adult who makes the decision about whether or not to participate in AFDC. In most situations, there is only one adult per AFDC household, while there may be several children, so the caseload correlates most closely with the number of decision makers. Lastly, there appears to be more political interest in understanding the factors that affect the number of cases than those that determine the number of recipients per se. In fact, most welfare reform waivers are designed to affect the caseload rather than the number of recipients.

**Table 1 Sensitivity of Static Estimates of the Impact of Welfare Reform and the Business Cycle on per Capita AFDC Recipients in the pre-TANF Period<sup>a</sup>**

Variable	Col. 1 <sup>b</sup>	2	3	4	5	6	7	8	9
Unemployment rate	3.092 (0.264)	2.882 (0.245)	2.647 (0.220)	3.216 (0.262)	3.620 (0.244)	3.007 (0.267)	-0.978 (0.457)	1.018 (0.235)	1.424 (0.266)
Any waiver	-5.450 (1.947)	-5.414 (1.815)	-8.183 (2.067)	-5.062 (1.921)	-6.327 (2.272)	-3.314 (2.091)	-1.451 (1.529)	-0.911 (1.181)	-0.123 (1.254)
% of 1993–1996 decline due to the economy	30.5	29.5	26.1	31.7	35.7	29.7	-9.7	10.0	14.1
% of 1993–1996 decline due to welfare reform	15.8	15.7	23.7	14.6	18.3	9.6	4.2	2.6	0.4

<sup>a</sup> All coefficients are multiplied by 100. Standard errors are in parentheses. The data are annual and pertain to all 50 states and the District of Columbia. Unless noted otherwise, all regressions are based on fiscal years 1976–1996, use total recipients, are weighted by the state population, use levels, and have controls for the real maximum benefit guarantee for a family of 3, state-specific fixed effects, state-specific trends, and year dummies.

<sup>b</sup> Col. 1 = CEA (1997) specification 2.

Col. 2 = AFDC caseloads

Col. 3 = cubic trend

Col. 4 = no AFDC benefits

Col. 5 = unweighted

Col. 6 = Blank (1998) sample period

Col. 7 = Ziliak et al. (1997) sample period

Col. 8 = first differences

Col. 9 = unweighted first differences

To test the sensitivity of the model estimates to the choice of dependent variable, we report the results from a model of AFDC caseloads in column 2 of Table 1. The estimates are nearly identical, especially for the welfare waivers, to those from the model with AFDC recipients reported in column 1. This indicates that the differences between the CEA and Ziliak et al. are not due to the use of a different dependent variable.<sup>6</sup>

### **Year Dummies versus Cubic Trend**

The next dimension that differentiates the models of the CEA and Ziliak et al. is in the way that they controlled for period-specific macroeconomic factors that are common to all states. The CEA used annual year dummies; Ziliak et al. used a cubic trend because they used monthly data, and rather than append 120 month dummies to the regression, they parameterized the national trends with a cubic polynomial in order to capture the fall (1987–1990), rise (1990–1993), and subsequent fall (1993–1996) in caseloads. It is not clear *a priori* whether the use of a cubic trend is likely to favor the business cycle or welfare reform relative to year dummies.

Column 3 reports the sensitivity of the model estimates to the use of a cubic trend rather than year dummies. Relative to column 1, it appears that the cubic trend imputes less of an effect to the economy and considerably more to welfare reform; that is, the fraction of the 1993–1996 decline attributable to the economy falls to 26 percent and the fraction attributable to welfare reform rises to 24 percent. This suggests that, if anything, the specification used by Ziliak et al. is likely to favor welfare reform relative to the economy. Since Ziliak et al. attributed a smaller effect to welfare reform relative to the CEA, it is clear that the choice of a cubic trend cannot explain the discrepancy in results.

### **Benefits versus No Benefits**

Unlike the studies by the CEA and Blank, Ziliak et al. did not include welfare benefit levels as a regressor because of the lack of suitable instruments that could deal with the possible simultaneity with reciprocity. It is sensible to think that while benefit levels might

explain welfare reciprocity, the size of the caseload might also affect the benefit level. Indeed, the simultaneity between welfare benefits and reciprocity has been shown by Figlio, Kolpin, and Reid (forthcoming), Gramlich and Laren (1984), and Shroder (1995). Nonetheless, it is instructive to examine the sensitivity of the estimated business-cycle and welfare-reform effects to the inclusion of welfare benefits, even though they may be endogenous.

Column 4 of Table 1 presents a reduced-form version of the base-case model in which welfare benefits are omitted. As shown, the estimated welfare-reform and business-cycle effects differ trivially whether one includes or omits welfare benefits. Again, this suggests that controlling for welfare benefits, even if endogenous but treated as if they are exogenous, does not lead to substantive differences between Ziliak et al. and the CEA.

### **Weighted versus Unweighted**

Both the CEA and Blank weighted their regression models; the CEA used total population and Blank used the population of women between the ages of 15 and 45 (under the assumption that the latter are more likely at risk for entering AFDC). On the other hand, Ziliak et al. did not weight their regression model. In general, weighting a regression model is recommended in situations of nonrandom sampling, nonspherical disturbances, or random parameter heterogeneity (Deaton 1997, pp. 67–73). Since all 50 states and the District of Columbia are represented in the data, they clearly are not subject to problems associated with nonrandom sample-selection bias. The disturbances may, however, be nonspherical, most likely in the form of heteroskedasticity (and serial correlation in the absence of controls for caseload dynamics). Nonetheless, it not clear that the only source of heteroskedasticity arises from population as assumed in the CEA and Blank. A more agnostic approach is to assume that the form of heteroskedasticity is unknown and to simply adjust standard errors using the Eicker-Huber-White correction.

Martini and Wiseman (1997) criticized the CEA for weighting by arguing that if states are viewed as “laboratories” for waiver experiments, then each state should be given equal weight. Indeed, we have no *a priori* reason to believe that a state’s population factored into

HHS's decision-making process for welfare waivers. Martini and Wiseman's argument suggests that the impact of waivers is homogeneous across states, and if so, then unweighted regression is superior to weighted regression on efficiency grounds. If, instead, we expect the responses to the experiments to be different across states, then weighting like that of the CEA and Blank produces consistent estimates when the parameter heterogeneity is unrelated to the other variables in the regression model (i.e., random coefficients). If this is a correct parameterization of the unobserved heterogeneity, then the usual weighted-least-squares standard errors are incorrect, although this obviously has no effect on the parameter estimates (Deaton 1997, p. 73). Indeed, as long as the model is correctly specified, there should be no significant difference between weighted and unweighted parameter estimates. If, however, the model is misspecified (e.g., through a lack of controls for caseload, business-cycle, and welfare-reform dynamics), then weighted and unweighted parameter estimates may diverge.

In column 5 of Table 1, we report unweighted business-cycle and welfare-reform estimates. Weighting by population in column 1 has the effect of reducing the estimated impact of both the business cycle and welfare waivers. The fraction of the 1993–1996 decline attributable to the economy rises from 31 to 36 percent, while the fraction attributable to welfare reform rises from 16 to 18 percent when moving from weighted to unweighted regression analysis. The downward effect that weighting has on the waiver estimates is most likely due to the fact that the larger states had both relatively smaller caseload declines and later (or no) pre-TANF waiver approvals. Likewise, these larger states also experienced less-pronounced reductions in their unemployment rates. Because the share of the caseload decline due to the business cycle and welfare reform between the weighted and unweighted models differs by about 17 percent, this suggests that the static model in Eq. 1 is misspecified. However, because the proportionate increase attributable to the economy and to welfare reform is nearly identical, weighting the regression model is not likely the primary source of difference between the CEA and Ziliak et al.

## Sample Period

The CEA's estimates were based on federal fiscal years 1976–1996, while Blank used 1977–1995 fiscal years and Ziliak et al. used 1987–1996 fiscal years (although the latter used monthly, not annual, data). Because many states did not receive a welfare waiver until 1995 or 1996, it is possible that ending the sample in 1995 (as Blank did) would lead to a lower welfare-reform estimate. However, her estimated business-cycle effect should be quite comparable to that of the CEA because both samples include the dramatic contraction and subsequent expansion of the 1980s. The sample used in Ziliak et al., on the other hand, included the entire pre-TANF waiver period (like the CEA) but misses the substantial cyclical movements of the late 1970s and early 1980s. This suggests that Ziliak et al. and the CEA should have comparable welfare-reform estimates, but that the Ziliak et al. business-cycle effect may be either dampened or strengthened relative to the CEA, depending on the relative changes in caseloads associated with economic fluctuations.<sup>7</sup>

Columns 6 and 7 in Table 1 present business-cycle and welfare-reform estimates for the sample periods used by Blank and Ziliak et al., respectively. In general, the results confirm prior expectations, especially with regard to the 1977–1995 sample used by Blank. The fraction of the decline attributable to the economy using Blank's sample period is about 30 percent, compared with the 31 percent in the base model (Column 1). Alternatively, the fraction due to welfare reform is a much lower 9.6 percent due to the termination of the sample at the same time that many states were still in the process of receiving waiver approvals. The results in column 7 based on the Ziliak et al. sample, however, are somewhat perverse, in that a negative 9.7 percent share is attributed to the economy and only a 4.2 percent share to welfare reform. In annual data, we expect the Ziliak et al. sample may yield a smaller share to the economy than the CEA, but not a negative share.<sup>8</sup>

The differences in the point estimates in columns 1 and 7 seem too pronounced to be explained simply by different aggregate macroeconomic conditions between 1976 and 1987. Indeed, further analysis indicates that the differences (at least for the welfare waivers) are explained to a large extent by three states: Florida, Iowa, and Michigan. Eliminating those three states yields welfare waiver coefficients

of  $-1.69$  for the CEA sample period and  $-2.22$  for the Ziliak et al. sample period. However, the coefficient on unemployment remains negative in the Ziliak et al. sample. What is special about Florida, Iowa, and Michigan that by eliminating them from the sample makes the welfare-waiver effects comparable across sample periods? From 1987–1992, these three states saw a 19 percent increase in the caseload, while the rest of the country saw a slightly larger increase of 22 percent. However, from 1976–1987, the two sets were much different: the three states saw an increase of 11 percent when the rest of the country saw a reduction of 7 percent. As a result, from 1976–1992, these three states saw an increase of 30 percent while the rest of the country saw an increase of 15 percent. From 1992–1996, the three states saw a decline of 16 percent while the rest of the country saw a decline of 12 percent. Put differently, Florida, Iowa, and Michigan saw a bigger deviation from trend in the welfare-reform years than did the rest of the country. This suggests that the sample period matters in the annual static model, but it is not clear whether the CEA's period is preferable to the Ziliak et al. period, because the differences in welfare-reform estimates are driven by a few states.

### **Levels versus First Differences**

The CEA model in Eq. 1 above is estimated in levels; however, Ziliak et al. estimated their model in first differences. It is important to note that asymptotically fixed effects and first differences should provide the same point estimates as long as the model is well specified. The two estimators could diverge if there is measurement error in the regressors or if there are misspecified dynamics either in the form of a nonstationarity in caseloads or in state dependence. Ziliak et al. presented evidence that nonstationarity in AFDC caseloads is likely to be a problem, especially at the monthly level but also in annual data; indeed, formal augmented Dickey-Fuller tests cannot reject the null hypothesis of a unit root at the 5 percent level. In many panel-data applications, nonstationarity is less problematic because the asymptotics are based on the cross-sectional dimension (e.g., Holtz-Eakin, Newey, and Rosen 1988, p. 1373). However, in the CEA and Ziliak et al. papers, the cross-sectional dimension was only 51, which is substantially less than “large” as typically implied in panel-data asymptot-



ics, and thus suggests that nonstationarity cannot be dismissed out of hand.

Columns 8 and 9 in Table 1 present weighted and unweighted first-difference estimates, respectively. Both specifications yield substantially lower point estimates relative to the levels models. The fraction of the 1993–1996 decline attributable to the economy falls to 10 (14) percent, while the fraction attributable to welfare reform falls to a meager 2.6 (0.4) percent in the weighted (unweighted) model. Because of the substantial difference in parameter estimates, this suggests that the static model in Eq. 1 suffers from some form of misspecification, whether it be nonstationarity, lack of controls for state dependence, measurement error, or some combination of the three. Ziliak et al. argued that the difference is due to nonstationarity and state dependence in caseloads; however, they did not address the possibility of measurement error. It may be the case that measurement error in state unemployment rates is exacerbated in the monthly data relative to annual and in first-differences relative to fixed effects as noted by Griliches and Hausman (1986). If that is the case, then Ziliak et al. should be biasing their estimates of the effect of the macroeconomy toward zero relative to the CEA by estimating the first-difference model with monthly data. This bias, however, is in the opposite direction to that argued by those who believe the Ziliak et al. model is somehow biased toward the macroeconomy. We do not formally address the issue of measurement error here, but instead proceed with the maintained assumption in CEA, Blank, and Ziliak et al., i.e., that unemployment rates are not measured with error.

### Static versus Dynamic Specifications

We now extend the static model in Eq. 1 to allow a detailed parameterization of dynamics. As shown in Ziliak et al., these dynamics are manifest both in the form of state dependence in caseloads and in lagged responses to cyclical movements in the economy. Specifically, even after controlling for heterogeneity in the form of state-specific fixed effects and trends, previous AFDC reciprocity may have a direct impact on future reciprocity, i.e., reciprocity may sluggishly adjust to changing economic and political conditions. In addition, we expect lagged unemployment to be important as well because welfare recipi-

ents are likely to be the last ones hired during an economic recovery and thus may not instantaneously move from welfare to work.

We consider two variants of the dynamic model, one in levels and the other in first differences. The dynamic levels estimating equation is

$$\text{Eq. 2} \quad R_{it} = \mu + \sum_{s=1}^s \rho_s R_{it-s} + \sum_{j=0}^J \alpha_j UR_{it-j} + \beta W_{it} \\ + \theta B_{it} + \gamma_t + \delta_t + \lambda_i t + \varepsilon_{it}$$

and the dynamic first-difference estimating equation is

$$\text{Eq. 3} \quad \Delta R_{it} = \sum_{s=1}^S \rho_s \Delta R_{it-s} + \sum_{j=0}^J \alpha_j \Delta UR_{it-j} + \beta \Delta W_{it} \\ + \theta \Delta B_{it} + \tilde{\gamma}_t + \lambda_t + \Delta \varepsilon_{it}$$

where all variables are defined as in Eq. 1 and where  $\tilde{\gamma}_t$  in Eq. 3 is a re-normalized vector of year effects. Notice that in Eq. 2 and 3 the lag lengths for reciprocity and the unemployment rate are not restricted to be the same. One can approach the issue of lag length either by starting broadly and then eliminating lags to improve model fit or by starting with a short lag structure and adding additional lags. We use the latter approach, in conjunction with the Schwarz criterion, and find that four lags of reciprocity rates and unemployment rates provides the best model fit.

In Table 2 we present the estimates of the dynamic models for a variety of specifications, including levels and first differences, weighted and unweighted, and the Ziliak et al. sample period. Column 1 presents weighted estimates of Eq. 2, which is the dynamic analogue to the weighted static CEA model in Table 1. The estimates reveal a strong degree of state dependence and lagged responses to changes in the unemployment rate. Important here is the change in the fractions of the decline attributable to the macroeconomy and to welfare reform in the dynamic context: we now attribute about 48 percent of the decline to the economy and -6.7 percent to welfare reform. The negative impact of welfare reform follows from the positive coefficient on "any waiver." A positive coefficient on welfare reform is not implausible if one considers that the variable "any waiver" is an aggregate of all waiver types,

**Table 2 Sensitivity of Dynamic Estimates of the Impact of Welfare Reform and the Business Cycle on per Capita AFDC Recipients in the pre-TANF Period<sup>a</sup>**

Variable	Levels models				First difference models			
	Col. 1	2	3	4	5	6	7	8
Recipients ( $t-1$ )	118.737 (3.542)	114.263 (5.332)	73.108 (6.828)	62.218 (7.933)	53.429 (3.638)	46.889 (5.077)	42.166 (7.010)	37.384 (7.534)
Recipients ( $t-2$ )	-51.848 (5.463)	-45.374 (6.192)	-37.426 (8.564)	-33.053 (8.459)	-15.044 (4.022)	-12.532 (4.559)	-19.022 (7.534)	-17.454 (7.333)
Recipients ( $t-3$ )	9.761 (5.275)	11.883 (5.165)	-0.937 (9.592)	3.368 (7.745)	5.456 (3.983)	8.277 (3.874)	-6.607 (8.825)	-1.954 (7.131)
Recipients ( $t-4$ )	-6.533 (3.265)	-9.856 (3.481)	-7.456 (8.380)	-6.545 (6.060)	-5.890 (3.270)	-8.168 (3.319)	-13.519 (8.828)	-8.679 (6.335)
Unemployment rate ( $t$ )	0.835 (0.216)	1.594 (0.275)	-0.443 (0.523)	-0.032 (0.428)	0.809 (0.219)	1.434 (0.281)	-0.534 (0.534)	0.014 (0.446)
Unemployment rate ( $t-1$ )	0.541 (0.262)	0.258 (0.297)	-0.055 (0.456)	0.159 (0.350)	1.105 (0.205)	1.259 (0.226)	-0.160 (0.460)	0.023 (0.350)
Unemployment rate ( $t-2$ )	-0.035 (0.259)	-0.108 (0.279)	0.433 (0.427)	0.521 (0.336)	0.603 (0.210)	0.611 (0.227)	0.671 (0.425)	0.659 (0.318)
Unemployment rate ( $t-3$ )	0.437 (0.253)	0.292 (0.270)	0.562 (0.419)	0.426 (0.340)	0.653 (0.202)	0.480 (0.215)	0.760 (0.427)	0.614 (0.372)
Unemployment rate ( $t-4$ )	0.393 (0.197)	0.527 (0.216)	0.623 (0.507)	0.807 (0.406)	0.712 (0.204)	0.581 (0.218)	0.993 (0.496)	0.872 (0.409)
Any waiver	1.056 (0.772)	-0.175 (0.906)	0.887 (0.927)	0.604 (1.088)	0.610 (0.929)	0.505 (1.274)	1.477 (0.974)	1.415 (1.194)

% of 1993–1996 decline due to the economy	47.5	56.4	18.3	30.5	68.9	75.5	22.9	30.7
% of 1993–1996 decline due to welfare reform	–6.7	1.1	–4.2	–2.8	–3.1	–2.5	–5.6	–5.7

<sup>a</sup> All coefficients are multiplied by 100. Standard errors are in parentheses. The data are annual and pertain to all 50 states and the District of Columbia. Unless noted otherwise, all regressions are based on fiscal years 1976–1996, use total recipients, are weighted by the state population, are in levels, and have controls for the real maximum benefit guarantee for a family of 3, state-specific fixed effects, state-specific trends, and year dummies.

<sup>b</sup> Col. 1 = base case

Col. 2 = unweighted

Col. 3 = Ziliak et al. sample period

Col. 4 = unweighted + Ziliak et al. sample period

Col. 5 = base case

Col. 6 = unweighted

Col. 7 = Ziliak et al. sample period

Col. 8 = unweighted + Ziliak et al. sample period

and a positive effect simply implies that the weighted impact of caseload-increasing waivers (e.g., higher earnings disregards and asset limits) dominates caseload-decreasing waivers. Ziliak et al., in their dynamic model of monthly data, disaggregated waiver types into work requirements, time limits, work incentives, and responsibility waivers and also permitted lag effects of waivers, yet still found that, for the nation as a whole, the economy accounts for 66 percent of the 1993–1996 decline and welfare reform for –9 percent. This suggests that the results in Table 2 are not an artifact of the aggregated “any waiver” specification. Consequently, controlling for dynamics in welfare reciprocity enhances the role of the economy and reduces the role of welfare reform in accounting for the decline in welfare utilization between 1993 and 1996.

We also reconsider several of the model specifications reported in Table 1; in particular, in the static model we found that there are differences depending on whether one weights the regression. In column 2 (Table 2) we report the results from the unweighted analogue to column 1. As in the static model, the contributions of both the macroeconomy and welfare reform increase relative to the weighted model, although the share attributable to welfare reform is effectively zero. We also noted that the results are sensitive to sample period. Hence, in columns 3 and 4, we present weighted and unweighted parameter estimates from the Ziliak et al. sample period. While the welfare reform effects are quite comparable (columns 3 and 4 relative to columns 1 and 2), the share of the decline attributable to the economy falls substantially.<sup>9</sup> This result underscores the potential pitfall of using a relatively short time horizon to identify business-cycle effects. Again, however, it is important to emphasize that this criticism does not apply directly to the Ziliak et al. paper, as they used 120 months rather than 10 years.

Lastly, we address the issue of levels versus first differences in columns 5 to 8. Recall that dynamics might arise not only from state dependence and lagged responses to unemployment rates, but also through nonstationarity. Columns 5 and 6 indicate that first differences increase the fraction of the decline attributable to the robust economy a further 45 percent over the col. 1 weighted model (to 69 percent) and by 34 percent over the col. 2 unweighted model (to 76 percent). Interestingly, though, the first-difference specifications do little to the wel-

fare reform estimates. Consequently, while the dynamic first-difference specification enables the model to identify a larger role for the macroeconomy relative to a dynamic levels model, this is not accomplished at the expense of welfare reform, but instead from other previously unobserved factors in the model (such as state-specific trends).<sup>10</sup>

In summary, we conclude from our reconciliation that the majority of the difference in model estimates between the CEA and Ziliak et al. arises from the treatment of dynamics. These dynamics surface in the form of nonstationarities in caseloads, sluggish adjustment of current caseloads to past caseloads, and lags in the response of caseloads to changes in unemployment rates. First-differencing to eliminate a possible nonstationarity permits the dynamic model to attribute a larger role to the macroeconomy relative to a static or dynamic levels model. However, after differencing the dynamic model, weighting the regression no longer has a substantive impact on the parameter estimates. The primary consequence of controlling for caseload dynamics is to reduce the role of welfare reform relative to the macroeconomy in accounting for the decline in AFDC reciprocity. Our preferred model specification indicates that the macroeconomy accounted for three-quarters of the 1993–1996 decline in welfare recipients, while welfare reform had a negligible impact.

## **WHAT WILL HAPPEN TO RECIPIENCY RATES IN THE NEXT RECESSION?**

A key issue confronting policymakers is how welfare caseloads might respond in the event of a recession. If AFDC cases only respond weakly to business-cycle conditions, then we would expect the welfare-program budget surpluses that many states have enjoyed recently to persist even into a recessionary period. Alternatively, if caseloads are strongly countercyclical, then states who have failed to save for a “rainy day” may face difficult fiscal constraints during the next cyclical downturn. Moreover, if the robust economy has fostered implementation of welfare reform, then when the economy turns toward recession this interrelationship may become disentangled. To address these

issues, we use several of the dynamic models from Table 2 to examine the responsiveness of reciprocity rates to alternative “shocks” to unemployment. We then investigate the extent to which the economy and welfare reform are interrelated and the implications of this link in the event of a recession.<sup>11</sup>

### **Dynamic Short-Run and Long-Run Simulations**

In Table 3 we present both short-run and long-run impacts of alternative unemployment rate increases on reciprocity rates. Specifically, based on the parameter estimates from the dynamic models in Table 2, we solve for the long-run, steady-state impact of the unemployment rate on reciprocity rates. We then simulate the impact of unemployment rate increases of 1 to 5 percentage points four years into the future; these simulations are possibly more reasonable estimates given that the long-run steady state is rarely attained. While our preferred model in Table 2 is the unweighted, dynamic first-difference column 6, we also present simulation results for the weighted and unweighted dynamic levels models (from columns 1 and 2, as well as the weighted, dynamic first-difference model in column 5).

The first column of Table 3 contains the implied long-run effect of a 1-percentage-point increase in the unemployment rate on welfare reciprocity. This effect ranges from 6.26 percent in the weighted, dynamic first-difference model to 8.81 percent in the unweighted, dynamic levels model. Interestingly, although the short-run effect of the unemployment rate on reciprocity is higher in the first-differences models relative to levels, the levels models imply a larger long-run effect because the adjustment to the long-run equilibrium is more attenuated in levels. Most important, however, the long-run equilibrium estimates underscore the importance of controlling for dynamics in modeling AFDC reciprocity. In the static models of Table 1, the short-run and long-run effects coincide. However, the estimates in Table 3 reinforce the fact that the static model is a misspecification, because the long-run estimates in levels are 2.5 times their static counterparts in Table 1, while the long-run estimates in first differences are 5–6 times the static estimates.

The remaining five columns in Table 3 present estimates four years into the future of increases of various magnitudes in the unemployment

**Table 3 Simulated Long-Run and Four-Year Impacts of Alternative Unemployment Rate Increases on Welfare Recipient Rates (%)**

Specification	Implied long-run effect of 1 p. pt. <sup>a</sup>	Four-year impact from an unemployment rate increase of				
		1 p. pt.	2 p. pt.	3 p. pt.	4 p. pt.	5 p. pt.
Weighted levels	7.29	3.7	7.4	11.1	14.8	18.5
Unweighted levels	8.81	4.4	8.8	13.1	17.5	21.9
Weighted 1st difference	6.26	5.3	10.7	16.1	21.4	26.8
Unweighted 1st difference	6.66	5.9	11.7	17.6	23.4	29.3

<sup>a</sup> p. pt. = percentage point(s).



rate. For example, after four years, the unweighted first-difference model predicts that a 1-percentage-point increase in the unemployment rate will lead to a 5.9 percent increase in welfare recipients, while a 3-percentage-point increase generates a 17.6 percent increase. In these simulations, the first-difference models yield a larger effect on recipients than the levels models. This arises because the first-difference models yield larger short-run effects relative to levels, and simulations based on a four-year time horizon are dominated by short-run influences. The simulations suggest that welfare caseloads are quite cyclically sensitive, and that if the economy were to make a substantive turn for the worse, many states may experience a surge in welfare recipients.

### **Interactions between Welfare Reform and the Macroeconomy**

An issue neglected up to this point is the potential role of an interaction between welfare reform and the robust economy since 1993 in fostering the rapid decline in AFDC caseloads. We address the possibility of interactions between welfare reform and the business cycle in the context of the dynamic levels and first-differences models in Eq. 2 and 3. Specifically, we consider interactions between the “any waiver” variable with the contemporaneous unemployment rate and then with the full set of current and lagged unemployment rates. If economic activity stimulates the caseload reductions associated with welfare reforms *and if this effect is independent of the “natural” relationship between the business cycle and the welfare caseload*, the coefficients on these interactions will be positive.<sup>12</sup>

In Table 4, we present estimates of the interaction between welfare reform and the macroeconomy on per capita AFDC recipients in the pre-TANF period. For ease of presentation, we suppress the coefficients on the lagged dependent variable and the current and lagged unemployment rates; we present the waiver coefficient along with the interactions. In addition, because the partial effect of welfare reform is dependent on the level of the unemployment rate, we compute the impact of welfare reform after four years in situations with a sustained unemployment rate of 2, 4, 6, or 8 percent. Finally, we also present the *p*-value on the (joint) significance of the interaction term(s); that is, for models with one interaction, the *p*-value refers to the *t*-statistic, while

for models with several interactions the  $p$ -value refers to a Wald test of the null hypothesis that the interactions are jointly zero.

In the weighted, dynamic levels model column 1, we confirm our prior expectation of a positive interacted effect between the macroeconomy and welfare reform.<sup>13</sup> This interaction is highly significant, with a  $p$ -value of 0.00. The model predicts that after four years, welfare reform leads to a 5.6 percent reduction in per capita recipients in states with an unemployment rate of 2 percent, while it leads to an increase of 2.8 percent in states with an unemployment rate of 8 percent. Comparable estimates are found in the fully interacted (column 2) as well as in the multiple-interaction difference specifications (columns 6 and 8), while evidence of a caseload-decreasing effect of welfare reform is less obvious in the unweighted levels models and the single-interaction first-difference specifications. Taken as a whole, the estimates in Table 4 suggest that pre-TANF welfare reform require a robust economy (i.e., low unemployment rates) in order to have a negative impact on reciprocity rates.

### **A Lagniappe**

While our primary focus in this paper is to provide a reconciliation between the CEA and Ziliak et al. estimates of the effect of welfare reform and the macroeconomy on per capita AFDC recipients in the pre-TANF period, there is much policy interest in understanding the sources of caseload declines after passage of PRWORA in August 1996. A difficulty in applying the model described here to the post-TANF period is correctly defining the welfare-reform variable, because the reform applies to all states (unlike the pre-TANF waiver programs). Nonetheless, one possible strategy is to use the date of waiver approval for those states that obtained waivers and to use the date of approval for the TANF plan for those states without waivers. We did this, and then updated our data to include observations from the 1997 and 1998 federal fiscal years and re-ran the dynamic levels and first-difference models in Eq. 2 and 3.

The estimates of the impact of the macroeconomy on reciprocity rates are nearly identical to those reported in Tables 2 and 3. For instance, the estimated long-run effect of a 1-percentage-point increase in the unemployment rate is 6.55 percent in both the weighted and

**Table 4 Estimates of the Interaction between Welfare Reform and the Business Cycle on per Capita AFDC Recipients in the pre-TANF Period<sup>a</sup>**

Variable	Levels models				First-difference models			
	Col. 1 <sup>b</sup>	2	3	4	5	6	7	8
Any waiver	-5.324 (2.359)	-10.787 (3.292)	-4.155 (2.822)	-4.497 (3.467)	-0.895 (3.162)	-5.427 (6.249)	0.312 (4.730)	-1.686 (6.137)
Any waiver $\times$ unemployment rate ( <i>t</i> )	1.013 (0.354)	-0.161 (0.998)	0.752 (0.476)	0.713 (1.078)	0.232 (0.466)	-1.080 (1.460)	0.035 (0.739)	0.426 (1.318)
Any waiver $\times$ unemployment rate ( <i>t</i> -1)		0.693 (1.492)		-1.328 (1.561)		-2.387 (2.709)		-5.039 (2.428)
Any waiver $\times$ unemployment rate ( <i>t</i> -2)		0.816 (1.324)		2.221 (1.376)		7.051 (2.611)		7.895 (2.410)
Any waiver $\times$ unemployment rate ( <i>t</i> -3)		0.390 (1.016)		-0.359 (1.129)		-0.983 (1.834)		-0.944 (1.975)
Any waiver $\times$ unemployment rate ( <i>t</i> -4)		-0.006 (0.678)		-0.523 (0.642)		-1.890 (1.361)		-2.055 (1.311)
<b>Percentage change in recipients after four years</b>								
Unemployment rate of								
2%	-5.6	-5.2	-4.5	-1.4	-0.2	-0.1	0.1	1.1
4%	-1.3	-2.7	-1.9	0.3	0.0	2.0	0.2	2.7
6%	0.8	-0.3	0.6	2.1	0.2	4.0	0.2	4.4
8%	2.8	2.2	3.2	3.9	0.4	6.0	0.2	6.0

Wald test of significance of interactions							
<i>P</i> -value	0.00	0.00	0.11	0.08	0.62	0.00	0.96

<sup>a</sup> All coefficients are multiplied by 100. Standard errors are in parentheses. The data are annual and pertain to all 50 states and the District of Columbia for fiscal years 1976–1996. Each regression controls for 4 lags of per capita recipients, 4 lags of unemployment rates, the real maximum benefit guarantee for a family of 3, state-specific fixed effects, state-specific trends, and year dummies.

<sup>b</sup> Cols. 1 and 2 = weighted                      Cols. 5 and 6 = weighted  
       Cols. 3 and 4 = unweighted                Cols. 7 and 8 = unweighted

unweighted dynamic differences model. However, the welfare-reform variable is uniformly negative in both the levels and differences models, although the effects are relatively small and statistically insignificant. These updated estimates are suggestive, though, that welfare reform has played a larger independent role on the decline in reciprocity rates in the post-PRWORA period. The finding of an enhanced welfare reform effect in the post-PRWORA period is fully expected, as our prior, stated in Ziliak et al. (1997), is that welfare reform should take more time to affect caseloads than the period covered in the CEA, Blank, and Ziliak et al. studies.

## CONCLUSION

Our reconciliation with the previous caseload literature suggests that the differing conclusions emanate largely from the treatment of dynamics. These dynamics surface in the form of nonstationarities in caseloads, sluggish adjustment of current caseloads to past caseloads, and lags in the response of caseloads to changes in unemployment rates. The primary consequence of controlling for caseload dynamics is to reduce the role of welfare reform relative to the macroeconomy in generating the decline in AFDC caseloads. Our preferred specification, an unweighted, dynamic first-difference model, predicts that the macroeconomy accounted for about 75 percent of the 1993–1996 decline in reciprocity rates, while the effect of welfare reform was negligible. We find that the implied long-run effect of a 1-percentage-point increase in the unemployment rate is 2.5 to 6 times the static estimate in levels and first differences. In addition, we find that reciprocity rates (caseloads) are quite cyclically sensitive: a 2-percentage-point increase in the unemployment rate leads to an 11.7 percent increase in welfare reciprocity after four years, while a 4-percentage-point increase yields a 23.4 percent increase in reciprocity.

Further underscoring the important role that the macroeconomy plays in determining caseloads, the analysis suggests that welfare reform efforts have been greatly aided by the simultaneous presence of a robust economy. Bishop (1998) presented evidence that most of the increase in labor force participation rates since 1994 are among single

women with children. If a large share of these women are former welfare recipients, then the results here suggest that the movement from welfare to work would be much weaker in a sluggish economy. However, even in the presence of economic growth, many welfare recipients may face substantial personal barriers to employment (Danziger et al. 1998).

This raises the broader task of delineating the goals of welfare reform. Reducing the caseload may be worthy in its own right if one's objective is to reduce the size of government spending, and the results here present evidence on the influence of the macroeconomy and welfare reform in achieving that goal. However, if the objective is to reduce poverty, then the results of this study do not directly speak to the outcomes of former welfare recipients. Unfortunately, many states are not following their former welfare cases; thus, a better understanding of welfare reform is incumbent upon correcting this deficiency.

## Notes

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1. For reasons discussed below, the replication of the CEA actually focuses on their specification 2 of Table 2, which predicts a 31 percent share of the decline to the business cycle and a 15 percent share to welfare reform.
2. To a lesser extent we reconcile the results with Blank (1998) as well. Blank focused primarily on the 1990–1993 run-up in caseloads and argued that the economy does not explain this unexpected run-up. Instead, she attributed the increase to a rise in child-only cases, an increase in take-up rates, and a long-term, yet unexplained, increase in eligibility.
3. See Peskin, Tapogna, and Marcotte (1993) for a complete list of these studies.
4. The Basic program, which comprises about 95 percent of total cases, consists of single parents (mainly women) and their children. The UP program permits both parents to be present, although the primary income earner must be under fiscal

stress, e.g., must work less than 100 hours in a month. The UP program was available only in about one-half of the states prior to the Family Support Act of 1988, which mandated all states to offer the program by 1990. However, HHS stopped making the distinction between the Basic and UP programs as of June 1997 because many states only maintain a single program under PRWORA.

5. The estimated welfare-waiver coefficient differs slightly from that reported in the CEA. The discrepancy arises from a miscoded waiver for West Virginia in the original CEA data, as noted in Levine and Whitmore (1998). The different coefficients, coupled with a slightly different weighting scheme, results in our simulations yielding a bit more of the share of the 1993–1996 decline to welfare reform than did the CEA.
6. Blank (1997) differed from the CEA and Ziliak et al. by conducting her analysis for the Basic and UP programs separately. The estimates reported here are very similar to those reported by Blank for the Basic program, but the UP program is much more cyclically sensitive. This suggests that examining total recipients is not misleading if one is interested in movements in the largest segment of the program or in forecasting aggregate recipients in general.
7. Importantly, though, Ziliak et al. actually attributed a larger share to the economy and a smaller share to welfare reform. This is partly due to their use of monthly data, which picks up high-frequency movements in the business cycle, and from the use of a dynamic model as described below.
8. In results not tabulated, we estimated column 7 without weighting the regression model and found the business cycle to have a small, but positive, share of the 1993–1996 decline in recipients.
9. The estimates in columns 3, 4, 7, and 8 are only meant to be suggestive, because the relatively short time horizon may make the coefficients of the lagged dependent variables susceptible to the so-called Nickell bias (Nickell 1981), that is, the bias (toward zero) in the lagged dependent variable that arises from the correlation between the lagged dependent variable and the model's error term. Ziliak et al. argued that this bias is negligible in their sample of monthly data since  $T = 120$ ; however, the annualized version of the Ziliak et al. sample in Table 2 only has  $T = 10$ . The CEA sample, however, has  $T = 21$  and thus again the Nickell bias is likely to be of smaller concern. The latter seems verified in that the results in columns 5 and 6 are quite similar to the results in Ziliak et al.
10. One further difference between Ziliak et al. and the CEA is that Ziliak et al. introduced a "time since waiver approval" variable. We examined a comparable specification in the context of the annual models here without any substantive change in the conclusions. If anything, the share attributable to welfare reform was more negative.
11. There might be some concern that with passage of PRWORA in August 1996, a structural change took place in the relationship between unemployment rates and welfare caseloads. If so, then out-of-sample forecasts based on pre-PRWORA data might be unreliable. We investigated this possibility in the context of a dynamic model of AFDC caseloads using state-level monthly data through March

1998. We interacted the five lags of the unemployment rate with a dummy variable that equaled 1 for any month after September 1996 (the sample began in October 1980) and could not reject the null hypothesis of no change in the unemployment rate coefficients after PRWORA. Hence, this suggests that there was no structural change in the relationship between unemployment rates and welfare caseloads.
12. It might be the case that tests of complementarities between the business cycle and welfare reform are better conducted within a state, as opposed to among states. The reason for this would be that within-state analyses offer a more natural experiment—the welfare reform policy should be relatively uniform within states (rather than among states) and other contemporaneous political and social factors are more likely to be constant within a state. This suggests that the tests conducted here are likely biased against finding complementarities.
  13. Levine and Whitmore (1997) found a statistically insignificant impact on the interacted term in their static model. We confirmed their result, but we also found that the interaction was strongly statistically significant without weights. This again underscores the likely misspecification of a static model.

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